THE MEANING

OF WAVE MECHANICS*

by

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Louis de Broglie's great theoretical discovery of the wave phenomenon associated with the electron was followed within a few years, on the one hand by incontrovertible experimental evidence (based on interference patterns) of the reality of the de Broglie waves (Davisson and Germer, G. P. Thomson), and on the other hand by a vast generalization of his original ideas, which embraces the entire domain of physics and chemistry, and may be said to hold the field today along the whole line, albeit not precisely in the way de Broglie and his early followers had intended.

For it must have given to de Broglie the same shock and disappointment as it gave to me, when we learnt that a sort of transcendental, almost psychical interpretation of the wave phenomenon had been put forward, which was very soon hailed by the majority of leading theorists as the only one reconcilable with experiments, and which has now become the orthodox creed, accepted by almost everybody, with a few notable exceptions. Our disappointment consisted in the following. We had believed that the eigenfrequencies of the wave phenomenon, which were in exact numerical agreement with the, until then so called, energy levels, gave a rational understanding of the latter. We had confidence that the mysterious "fit and jerk theory" about the jump-like transition from one energy level to another was now ousted. Our wave equations could be expected to describe any changes of this kind as slow and actually *describable* processes. This hope was not informed by personal predilection for continuous description, but if anything by the wish for any kind of description at all of these changes. It was a dire necessity. To produce a coherent train of light waves of $100cm$ length and more, as is observed in fine spectral lines, takes a time comparable with the average interval between transitions. The transition must be coupled with the production of the wave train. Hence if one does not understand the transition, but only understands the "stationary states", one understands nothing. For the emitting system is busy all the time in producing the trains of light waves, it has no time left to tarry in the cherished "stationary states", except perhaps in the ground state.

N. B. – Selon le souhait exprimé par l'auteur, nous donnons son texte original en face de la traduction. (Translation omitted in this reprint.)

* Reprinted from: *LOUIS DE BROGLIE, Physicien et Penseur*, ÉDITIONS ALBIN MICHEL, 22, RUE HUYGHENS, Paris, 1952, pgs. 16–30. Originally given as a talk at the July Colloquium, Dublin Institute for Advanced Stidies, 1952. The two texts are identical.

Another disconcerting feature of the probability interpretation was and is that the *wave function* is deemed to change in two entirely distinct fashions; it is thought to be governed by the wave equation as long as no observer interferes with the system, but whenever an observer makes a measurement, it is deemed to change into an eigenfunction of *that* eigenvalue of the associated operator that he has measured. I know only of one timid attempt (J. von Neumann in his well known book) to put this "change by measurement" to the door of a perturbing operator introduced by the measurement, and thus to have it also controlled solely by the wave equation. But the idea was not pursued, partly because it seemed unnecessary to those who were prepared to swallow the orthodox tenet, partly because it could hardly be reconciled with it. For in many cases the alleged change involves an *actio in distans*, which would contradict a firmly established principle, if the change referred to a physical entity. The non-physical character of the wave function (which is sometimes said to embody merely our knowledge) is even more strongly emphasized by the fact that according to the orthodox view its change by measurement is dependent on the observer's taking cognizance of the result. Moreover the change holds only for the observer who does. If *you* are present, but are not informed of the result, then *for you* even if you have the minutest knowledge both of the wave function before the measurement and of the appliances that were used, the changed wave function is irrelevant, not existing, as it were ; *for you* there is, at best, a wave function referring to the measuring appliances plus the system under consideration, a wave function in which the one adopted by the knowing observer plays no distinguished rôle.

M. de Broglie, so I believe, disliked the probability interpretation of wave mechanics as much as I did. But very soon and for a long period one had to give up opposing it, and to accept it as an expedient interim solution. I shall point out some of the reasons why the originally contemplated alternative seemed deceptive and, after all, too naive. The points shall be numbered for later reference; the illustrating examples are representative of wide classes.

i) As long as a particle, an electron or proton etc., was still believed to be a permanent, individually identifiable entity, it could not adequately be pictured in our mind as a wave parcel. For as a rule, apart from artificially constructed and therefore irrelevant exceptions, no wave parcel can be indicated which does not eventually disperse into an ever increasing volume of space.

ii) The original wave-mechanical model of the hydrogen atom is not self-consistent. The electronic cloud effectively shields the nuclear charge towards outside, making up a neutral whole, but is inefficient inside; in computing its structure its own field that it will produce must not be taken into account, only the field of the nucleus.

iii) It seemed impossible to account for e.g. Planck's radiation formula without assuming that a radiation oscillator (proper mode of the hohlraum) can only have energies $nh\nu$, with n an integer (or perhaps a half odd integer). Since this holds in all cases of thermodynamic equilibrium that do not follow the classical law of equipartition we are thrown back to the discrete energy states with abrupt transitions between them, and thus to the probability interpretation.

iv) Many non-equilibrium processes suggest even more strongly the "transfer of whole quanta"; the typical, often quoted example is the photoelectric effect, one of the pillars of Einstein's hypothesis of light quanta in 1905.

All this was known 25 years ago, and abated the hopes of "naive" wave-mechanists. The now orthodox view about the wave function as "probability amplitude" was put forward and was worked out into a scheme of admirable logical consistency. Let us first review the situation after the state of knowledge we had then. The view suggested by (iii) and (iv), that radiation oscillators, electrons and similar constituents of observable systems always find themselves at one of their respective energy levels except when they change abruptly to another one handing the balance over to, or receiving it from, some other system, this view, so I maintain, is in glaring contradiction with the above mentioned scheme in spite of the admirable logical self-consistency of the latter. For one of the golden rules of this scheme is, that any observable is always *found* at one of its eigenvalues, when you measure it, but that you must not say that it *has* any value, if you do not measure it. To attribute sharp energy values to all those constituents, whose energies we could not even dream of measuring (except in a horrible nightmare), is not only gratuitous but strictly forbidden by this rule.

Now let us review the situation as it is today. Two new aspects have since arisen which I consider very relevant for re-considering the interpretation. They are intimately connected. They have not turned up suddenly. Their roots lie far back, but their bearing was only very gradually recognized.

I mean first the recognition that the thing which has always been called a particle and, on the strength of habit, is still called by some such name is, whatever it may be, certainly *not* an individually identifiable entity. I have dwelt on this point at length elsewhere ["Endeavour", Vol. IX, Number 35, July 1950; reprinted in the Smithsonian Institution Report for 1950, pp. 183–196; in German "Die Pyramide", Jan. and Feb. 1951 (Austria)]. The second point is the paramount importance of what is sometimes called "second quantization".

To begin with, if a particle is not a permanent entity, then of the four difficulties labeled above, (i) is removed. As regards (ii), the quantization of de Broglie's waves around a nucleus welds into one comprehensive scheme all the 3n-dimensional representations that I had proposed for the n-body problems. It is not an easy scheme, but it is logically clear and it can be so framed that only the *mutual* Coulomb energies enter.

As regards (iii) – keeping to the example of black body radiation – the situation is this. If the radiation is quantized each radiation oscillator (proper mode) obtains the frequencies or levels $nh\nu$. This is sufficient to produce Planck's formula for the radiation in a cavity surrounded by a huge heat bath. I mean to say, the level scheme suffices: it is not necessary to assume that each oscillator *is* at one of its levels, which is absurd from any point of view. The same holds for all thermodynamical equilibria. I have actually given a general proof of this in the last of my "Collected Papers" (English version: Blackie and Son, Glasgow 1928). A better presentation is added as an appendix to the forthcoming 2$^{\text{nd}}$ impression of "Statistical Thermodynamics" (Cambridge University Press).

Under (iv) we alluded to a vast range of phenomena purported to be conclusive evidence for the transfer of whole quanta. But I do not think they are, provided only that one holds on to the wave aspect throughout the whole process. One must, of course, give up thinking of e.g. an electron as of a tiny speck of something moving within the wave train along a mysterious unknowable *path*. One must regard the "observation of an electron" as an *event* that occurs within a train of de Broglie waves when a contraption is interposed in it which by its very nature cannot but answer by discrete responses: a photographic emulsion, a luminescent screen, a Geiger counter. And one must, to repeat this, hold on to the wave aspect throughout. This includes, that the equations between frequencies and frequency differences, expressing the resonance condition that governs wave mechanics throughout, must *not* be multiplied by Planck's constant h and then interpreted as tiny energy balances of microscopic processes between tiny specks of something that have, to say the least, no permanent existence.

This situation calls for a revision of the current interpretation, which revels in computing transition probabilities from level to level, and disregards the fact that the wave equation, with few exceptions if any, indicates nothing of the sort, but leads each of the reacting systems into a state composed of a wide spread of energy eigenstates. To assume that the system actually leaps into just one of them which is selected by "playing dice", as it were, is not only gratuitous, but as was pointed out above, contradicts in most cases even the current interpretation. These inconsistencies will be avoided by returning to a wave theory that is not continually abrogated by dice-miracles; not of course to the naive wave theory of yore, but to a more sophisticated one, based on second quantization and the non-individuality of "particles". Originating from contraptions that by their very nature cannot but give a discrete, discontinuous response, the probability aspect has unduly entered the fundamental concepts and has domineeringly dictated the basic structure of the present theory.

In giving it up we must no longer be afraid of losing time-honoured atomism. It has its counterpart in the level-scheme (of second quantization) and nowhere else. It may be trusted to give atomism its due, without being aided by dice-playing.

To point here to the general failure of the present theory to obtain finite transition probabilities and finite values of the apparent mass and charge, might seem a cheap argument – and a dangerous one at that. The obvious retort would be: Can you do better, sir? Let me frankly avow that I cannot. Still I beg to plead that I am at the moment groping for my way almost single-handed, as against a host of clever people doing their best along the recognized lines of thought.

But let me still draw attention to a point that is seldom spoken of. I called the probability interpretation a scheme of admirable logical consistency. Indeed it gives us a set of minute prescriptions, not liable ever to be involved in contradiction, for computing the probability of a particular outcome of any intended measurement, given the wave function and the hermitian operator associated with that particular measuring device. But, of course, an abstract mathematical theory cannot possibly indicate the rules for this *association* between operators and measuring devices. To describe one of the latter is a long and circumstantial task for the experimentalist.

Whether the device which he recommends really corresponds to the operator set up by the theorist, is not easy to decide. Yet this is of paramount importance. For a measuring appliance means now much more than it did before the advent of quantum mechanics and of its interpretation which I am opposing here. It has a physical influence on the object; it is deemed to press it infallibly into one of the eigenstates of the associated operator. If it fails to put it in an eigenstate belonging to the value resulting from the measurement, the latter is quantum-mechanically not repeatable. I cannot help feeling that the precariousness of the said association makes that beautiful, logically consistent theoretical scheme rather void. At any rate its contact with actual laboratory work is very different from what one would expect from its fundamental enunciations.

A further discussion of the points raised in this paper can be found in a forthcoming longer (but equally non-mathematical) essay in the British Journal for the Philosophy of Science.

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